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The failure of research to affect educational practices has provoked discussion of the processes of research (finding the right kinds of questions to study) and development (learning how to apply knowledge), of the priorities of "basic" versus "applied" research. T. R. McConnell believes that problems can be selected which have both theoretical and practical relevance--a combination appealing to government-sponsored research and development centers. College-sponsored offices of institutional research have traditionally been concerned with operational problems although results of such research have limited or no relevance to academic problems and purposes. The fact that colleges fail to adopt policies or practices of verified worth indicates the need for utilizing knowledge produced by research. Government and private agencies have recognized that development is as important as research in effecting change. Evaluation of development is also necessary although difficult when goals cannot be tangibly measured. Research and development and evaluation considered as a total process may be the most promising approach to improvement in higher education. (JS)

WHEN WILL RESEARCH IMPROVE EDUCATION

by

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hen Will Research Improve Education?

*History records no precedent to this extraordinary adulation of research unless it be that of the Middle Ages to pilgrimage. When it prevailed, I doubt if anyone who was impelled to save his soul by going to a distant shrine was ever restrained by his friends. He would take off his shoes, pick up his stick, and go forth with everyone's blessing, just as today the research-bent abandons his occupation, picks up a box of index cards, and is on his way with shining eyes and a two-year grant amid general admiration. The very phrase "do research" shows that it is the act, not the goal, that matters, and though not many think of research as saving their souls, society at large does believe that there is salvation in it.*1

--JACQUES BARZUN

Although Barzun may doubt that research will ve education, most educators appear to be on the side o ociety at large" in believing that there is "salvation in it." lleges across the land are instituting self-studies at an unecedented rate; offices of research are becoming the new feature in the organization charts of institutions of higher ucation; the United States Office of Education Bureau of esearch budget is almost $100 million per year. The estion for many is not *whether* research will improve edution, but *when*.

Our ancestors of a century ago would find our world, with automobiles, television, spaceships, electric dishwashers, ozen foods, and miracle fabrics, virtually incomprehensible them. But they would probably find little difficulty in adsting to today's classroom. Apparently, the knowledge inustry has changed everything except education itself.

The failure of education to improve itself is certainly not ue to complacent satisfaction. One has only to peruse me titles at the corner newstand to recognize that schools and colleges are a favorite target for attack. Even the professional literature is replete with discussions of what education is not accomplishing. Recently, attention has been given to some analyses of why research has had so little impact upon educational practice and, more importantly, what can be done about it.

These questions are of vital concern to those who support research, to those who plan programs of research, and to those who would use research knowledge for the improvement of higher education. Since the *Research Reporter* is addressed to all those who seek ways to make education a more potent force in the lives of individuals and society, a review of the issues at stake may prove useful.

Essentially the discussions center around two processes--research and development. In other words, improvement in education will take place *when* we study the right kinds of questions and *when* we learn how to apply research knowledge to the practice of education.

THE NATURE OF THE RESEARCH.

The extreme of the positions regarding the nature of educational research are vividly portrayed by Henry Dyer (1966), vice president of Educational Testing Service, who contrasts the viewpoints of Nevitt Sanford and John Dale Russell regarding the role of institutional research. Sanford suggests that the research that will lead to improvements in practice will be characterized by "intensive, theoretically-oriented, long-term studies of students and intensive, probably also long-term studies of the inner workings of educational institutions." Departments handling such studies should be "free or relatively free of demands from its host institution for information relevant to its immediate problems." In contrast, John Dale Russell defines an office of institutional research as "an agency . . . attached directly to . . . the office of the president or executive vice president; it is assigned specific responsibility for carrying on studies needed for the making of important decisions about policy and procedures; and it works toward the primary goal of finding out how to save money that can be used to better advantage."

1 Barzun, Jacques. *Science: The glorious entertainment.* ew York: Harper and Row, 1964.

Until very recently, institutional researchers concerned themselves almost solely with applied research and left the basic research to the social scientists on the faculty. Now, offices of institutional research have developed enough sophistication to worry about whether their university will support "basic" research; and the social scientists, newly attracted to research and development centers, are finding themselves charged with the application of research to social problems. Thus the issue of whether research is "basic" or "applied" has taken on new significance for a wide variety of educational researchers.

T. R. McConnell

While few would deny the importance of theoretically oriented research, there is considerable difference in the priority which leaders in educational research would grant it. Lee J. Cronbach, professor of education and psychology at Stanford University, argues that the *only* research that will have any enduring impact upon education is that which involves a basic understanding of learning and motivation. He does not want to see the resources of the university diluted in the search for practical solutions to the educational problems of the moment. While he is content to let "others work on stopgap empirical solutions," he leaves no doubt about his conviction that long-range improvement in education lies in the search for explanatory principles (Cronbach, 1966). T. R. McConnell, professor of higher education at the University of California at Berkeley and founder of the Center, while agreeing with the importance of the search for explanatory principles of learning, notes that some theoretical positions, for example, Skinnerian learning theory, are extremely limited as a source of educationally fruitful investigations. Likewise, some practical problems in education, admittedly in need of solution, have little or no contribution to make to the deeper understandings which will lead to long-range improvement in education. The reconciliation, McConnell suggests, is to be found in the selection of educational problems which have *both* theoretical reference and practical relevance. He makes his recommendations for fruitful educational research after some careful observations of research which has "paid off" in other fields. In the natural sciences, for example, some fundamental scientific discoveries have lead to undreamed of future applications. It is also true that "there are striking instances in which applied research has contributed to scientific knowledge or stimulated basic investigation."[2]

While the combined virtues of practical and theoretical relevance may be the answer for the government-financed research and development centers, what about the college-sponsored offices of institutional research? What kind of research will help them contribute to the maximum development of their colleges?

Typically, offices of institutional research have been concerned with the operational nuts-and-bolts problems involving the allocation of resources. Dyer (1966) admits that offices of institutional research would neglect operational problems at their peril, but he warns that "research that does not go beyond the instrumentalities of education is unlikely to make any fundamental difference in the impact of the institution on its consituents or on the community it hopes to serve. Operational research, uninformed by theory, goes nowhere ... it may be concerned with ways of ensuring the solvency of the institution, but it does not ask what purposes are being served by keeping the institution solvent. It may be concerned with improving selection procedures so as to minimize the number of drop-outs, but it does not examine the question of whether admitting some potential drop-outs might not conceivably improve the quality of the student body and the educational experience."

Dyer's observations about the limited impact of operational research on education are verified in a recent report by Ernest Boyer, vice chancellor for university-wide activities for the State University of New York (1967). After a look at the influence of institutional research on the policies and practices of colleges and universities, he concludes that institutional research has certainly helped administrators toward a more orderly and efficient management of their *nonacademic* affairs but that it has had practically no effect on the heart of the educational enterprise — the academic program.

There is a good deal of reality in Charles Kettering's definition of research as "an organized method of finding out what you are going to do when you can't keep on doing what you are doing now." Since we are virtually at that point in education, the integration of the practical and the theoretical in research efforts seems to make sense — whether at the national or at the local level. Realistically, offices of institutional research will undoubtedly continue to work on operational problems, hopefully casting them in a framework that will lead to long-range as well as short-range solutions. We may also expect that the research and development centers will ground their investigations in reality but will continue the search for explanatory principles which will provide the basis for fundamental improvements in educational practice.

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2 Professor McConnell's remarks were presented in the American Educational Research Association and Phi Delta Kappa Award Lecture as the AERA and Phi Delta Kappa saluted him for "distinguished contributions to educational research." The reprint of the Phi Delta Kappa monograph is available upon request from the Center.

Perhaps a strong combination of forces is to be found in some sharing of responsibilities — with the national centers contributing some hypotheses for test and the local offices providing the proving grounds.

THE NATURE OF DEVELOPMENT

The production of knowledge about education is only a partial answer to improved practice; the appropriate utilization of knowledge is also essential. Many are convinced that educators are like the grizzled farmer who stopped the enthusiastic young extension agent from citing more of the latest agricultural research by saying, "Look, Sonny, I don't farm half as good as I know how now." The answer to improved education may well lie in the development and application of what we already know.

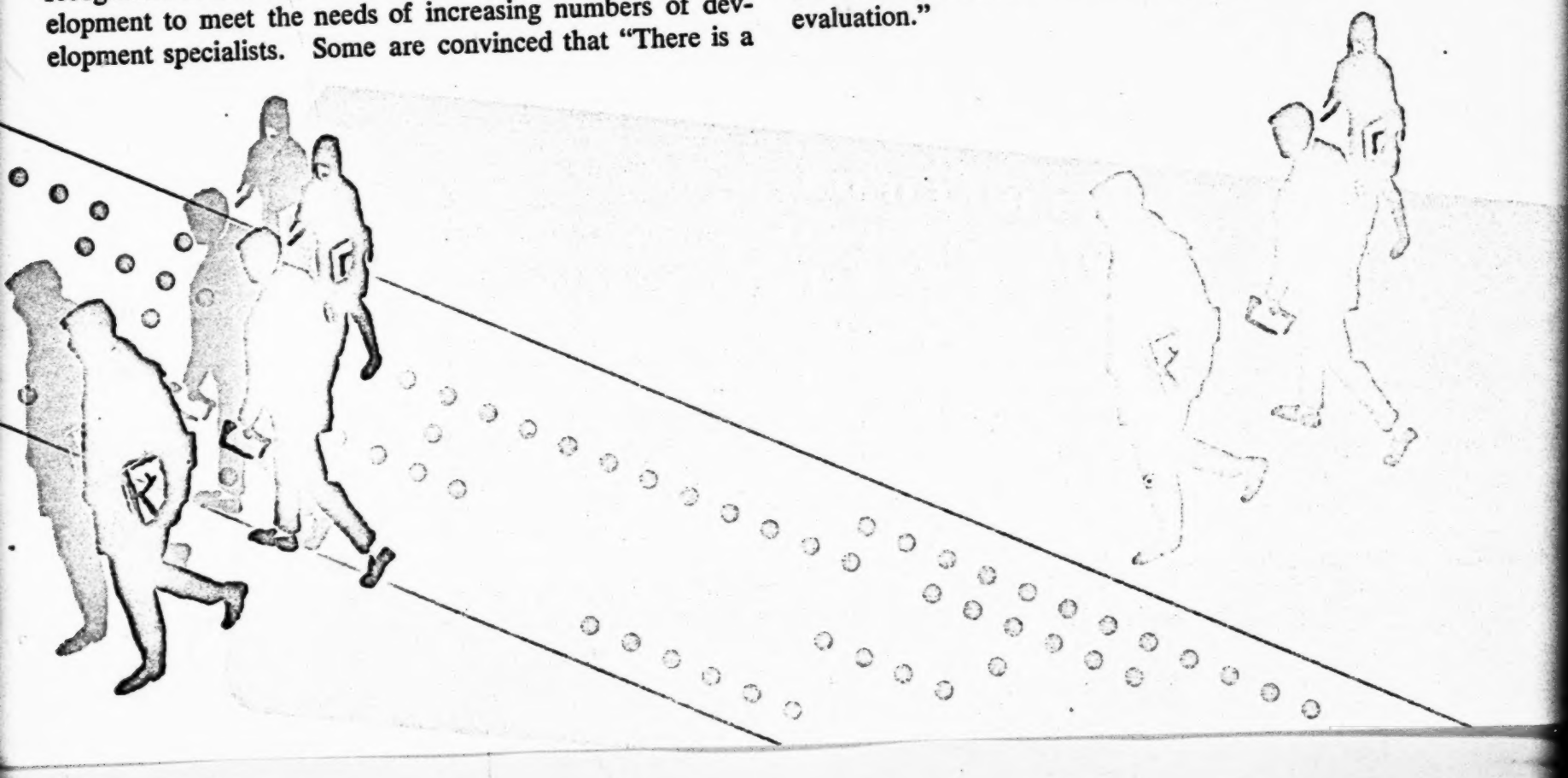
Why, one might ask, are we so tied to the credit-hour system despite the overwhelming evidence that it has little value as a measure of education and, worse yet, stands as a deterrent to flexible programs of education? Why, despite all of the very practical, obviously applicable research on advanced placement, do we still have colleges requiring all freshmen to start at the college's starting point rather than at the student's? Why are we so addicted to the class lecture, despite research showing equally good, and frequently better, alternatives? There seems to be no escape from the conclusion that we don't educate as well as we know how.

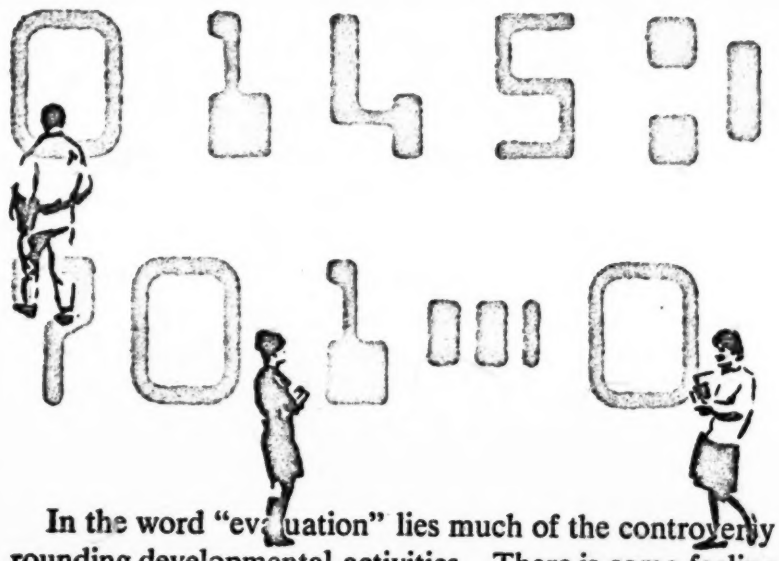
The U. S. Office of Education is one of the most visible proponents of development. The nine national research and development centers have a responsibility, not only for research, but for its development into useable educational practice as well. Some 20 regional laboratories, also established by the Office of Education, have, as their *primary* goal, the conversion of research knowledge into materials or concepts which can be used in schools throughout the country.

The growing conviction that development is as important as research in effecting change in education is spawning a new breed of professionals. A petition is presently before the American Educational Research Association requesting recognition of a new division on Research Utilization and Development to meet the needs of increasing numbers of development specialists. Some are convinced that "There is a tremendous gap between knowledge production and knowledge utilization that cannot be spanned *either* by the producer or by the utilizer himself, or even by these two acting in concert . . . (Guba, 1967)." In order to build a bridge between new knowledge and its social use, many are calling for the *training* of development specialists (Guba, 1967; Leavitt, 1967; and Mackie and Christensen, 1967).

Perhaps a new kind of "change agent" is needed. An observer of the higher education scene would have to admit, however, that neither producers nor potential users of educational knowledge have exhausted the possibilities for working together. Institutional researchers have been "curiously ambivalent" about their role in making recommendations and pointing out the implications of their studies, Boyer says, and he urges researchers to become direct agents of institutional change. At the same time, however, he faults the academic community at large for its failure to "examine seriously the proposition that a constructive approach to change — a determination to guide and develop it in orderly channels — holds immense promise; that it is, indeed, the key whereby educators may accomplish their fondest dreams and highest purposes."

There seems little doubt that attention must be given to the developmental process if research knowledge is to be utilized. Whether "development specialists" are needed or not remains open to question. Some Department of Defense research indicates that when a research idea is successfully converted into practice, it is most often by the same people and in the same laboratory involved in the original discovery (Carter, 1966). While the Center has a department whose responsibility it is to give particular attention to the utilization of research knowledge, some of the research staff participate in developmental activities, and some of the development staff engage in research. It is felt that this blending of efforts embodies the concept of the *process* involved in the utilization of research knowledge. McConnell has written, "The transition from research to practice is not one leap. It is a process, a flow, from basic through applied investigation, to invention and development, to innovation or production, and finally to evaluation."





In the word "evaluation" lies much of the controversy surrounding developmental activities. There is some feeling that development may outrun research in an overenthusiastic rush to innovate for innovation's sake. Cronbach maintains that we don't yet know enough about the educational process to institute nationwide changes, while McConnell warns, "Without evaluation, development may easily become quackery."

The amount of development effort needed varies greatly with the nature of the research question. The operational aspect of institutional research which has shown considerable impact on management practices is easily developed since it furnishes a direct answer to a specific question. Unfortunately, the questions that will make a basic difference in education are not easy to formulate. We cannot expect the search for improvements in education to be simple, nor the "payoff" to be quick. Dyer maintains that it is the *measureable* goals in education that affect practice. And one has only to observe our present criteria for quality education to recognize the truth of his statement — the number of books in the library, the number of Ph.D.'s on the faculty, the number of National Merit Scholars in the entering class. Although these are secondary goals at best, they do in fact serve as a target for colleges simply because they are easily measured. Until we can define goals such as "personal fulfillment," "responsible citizenship," etc. in a manner that will enable us to know when we are "on target," we are unable to do much about them.

Within the past decade, social scientists have become much more sophisticated in divising measures of important variables in the learning environment.[3] Volume II, Number 2, of the *Research Reporter* presented some research findings showing that students with creative potential (as measured by the Omnibus Personality Inventory) tend to drop out of college to a greater extent than do other students (Cross, 1967). A college which is losing large numbers of such students might, for example, take the goal of reducing the number of potentially creative dropouts as a measureable aspect of the vaguely defined goal of "assisting students to self-fulfillment." McConnell proposes that it might be worthwhile for some institutions to "indentify such students quickly, free them from rigid requirements and sequences, provide opportunities for creative expression or scientific inquiry from the beginning, and to make other adaptations that might encourage these students to realize their potentialities in generous degree."

This example illustrates the *process* of research and development. Through research, some characteristics of potentially creative students have been defined; a measure has been devised to identify such students; further research has revealed that large numbers of such students do not now attain fulfillment in our institutions of higher education; a specific and measureable institutional goal of reducing the dropout rate for these students can be established; a program can be developed which is hypothesized to come closer to meeting the self-fulfillment needs of these students than presently exists; and the result can be evaluated. Through long-term research, we can determine whether these students, tentatively identified as "creative," do in fact make creative contributions to the society in which they live. And finally, this process may assist us in constructing some theories about student development.

It appears that the federal government supports the concept of research and development in this *connected sense* (Boyan, 1967). It is possible, of course, to stop the flow of research and development at almost any point in the above example by protesting that we don't *know* that such students will make creative contributions or that we don't *know* that our program as developed will help them toward personal fulfillment. But as Dyer reminds us, "most of what (we) do as educators is in fact based on blind assumptions that could turn out to be *wrong*." The weight of the evidence to date is that a careful program of research and development and evaluation is our most promising approach to improvements in higher education.

K. PATRICIA CROSS

3 The work of Pace and Stern on college environments, Educational Testing Service's College Student Questionnaires, and the Center's Omnibus Personality Inventory are examples.

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